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Changes in Hospital Mortality Associated with Residency Work-Hour Regulations

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Background: In 2002, the Accreditation Council on Graduate Medical Education enacted regulations, effective 1 July 2003, that limited work hours for all residency programs in the United States.

Objective: To determine whether work-hour regulations were associated with changes in mortality in hospitalized patients.

Design: Comparison of mortality rates in high-risk teaching service patients hospitalized before and after July 2003, with nonteaching service patients used as a control group.

Setting: 551 U.S. community hospitals included in the Healthcare Cost and Utilization Project's Nationwide Inpatient Survey between January 2001 and December 2004.

Patients: 1 511 945 adult patients admitted for 20 medical and 15 surgical diagnoses.

Measurement: Inpatient mortality.

Results: In 1 268 738 medical patients examined, the regulations were associated with a 0.25% reduction in the absolute mortality rate ($P = 0.043$) and a 3.75% reduction in the relative risk for death. In subgroup analyses, particularly large improvements in mortality were observed among patients admitted for infectious diseases (change, −0.66%; $P = 0.007$) and in medical patients older than 80 years of age (change, −0.71%; $P = 0.005$). By contrast, in 243 207 surgical patients, regulations were not associated with statistically significant changes (change, 0.13%; $P = 0.54$).

Limitations: Teaching status was assigned according to hospital characteristics because direct information on each patient's provider was not available. Results reflect changes associated with the sum of regulations, not specifically with caps on work hours.

Conclusions: The work-hour regulations were associated with decreased short-term mortality among high-risk medical patients in teaching hospitals but were not associated with statistically significant changes among surgical patients in teaching hospitals.

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For more than 20 years, the Accreditation Council on Graduate Medical Education (ACGME) and other observers have been concerned with excessive resident duty hours (1–9). The ACGME imposed national restrictions on duty hours in 1987, New York State enacted hours restrictions in 1989, and the ACGME's Residency Review Committee for Internal Medicine adopted duty-hour limits in the 1990s. Despite these regulations, enforcement before 2003 was not stringent and excessive resident work hours were common. In part to avoid federal legislation, the ACGME approved new resident duty-hour regulations that became effective on 1 July 2003 (1). The regulations limited resident workweeks to 80 hours or fewer and limited continuous duty to 24 hours, with 6 additional hours for transfer of care.

The effect of these regulations is uncertain. An hours cap might reduce errors and mortality due to resident weariness. Alternatively, they might increase errors if they disrupt the continuity of patient care. Scholarly work on this question has been limited, and the conclusions of these studies were mixed. One study found that New York State's regulations did not affect mortality in patients who were admitted for pneumonia, congestive heart failure, or acute myocardial infarction (10). However, duty-hour violations were still common during the study period, making the results less relevant to current regulations. Another study indicated that interns working under a duty-hours cap made fewer errors than did a similar group under a traditional call system (6). This finding suggested that duty-hours caps improve patient outcomes, but the sample size was too small to show effects on mortality.

The regulations may not have altered outcomes if residency programs did not change working conditions. However, the official ACGME report and multiple program director surveys documented major changes in work hours in residency programs after July 2003 (2, 11–16). In addition, an independent survey reported that interns' average weekly work hours decreased from 70.7 to 66.6 hours despite widespread violations (5).

We retrospectively analyzed discharge data collected between 2001 and 2004 to determine whether the ACGME regulations were associated with changes in inpatient mortality. Unlike previous work, our study has the statistical power to detect small effects because we used a large, nationally representative data set with patients from nonteaching hospitals as a control group.

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Context

The health effects of regulations restricting housestaff work hours to 75 hours per week are largely unknown.

Contribution

The authors measured inpatient mortality of 1 268 738 patients admitted to 551 hospitals with medical diagnoses and 243 207 patients admitted for surgical diagnoses before and after 2003, when the work-hour regulation took effect. After 2003, the mortality rate for medical patients—but not surgical patients—decreased more in teaching hospitals than nonteaching hospitals.

Caution

The authors classified a hospital as "teaching" if it had a residency program; however, some teaching hospitals have nonteaching patients.

Implication

Limiting residents' work hours was associated with lower in-hospital mortality rates for medical patients in teaching hospitals.

—The Editors

METHODS

Data Sources

We used the Nationwide Inpatient Sample (NIS) from the Healthcare Cost and Utilization Project to assemble a nationally representative data set of hospital patients between 2001 and 2004. The NIS sampled approximately 20% of all community hospitals in the United States and included at least 7.4 million discharges annually since 2001. The NIS provided clinical and demographic information on each discharged patient, including age, sex, 10 diagnoses (1 principal and 9 secondary), principal procedure, month of admission, and income quartile of the patient's ZIP code. The NIS coded procedures and diagnoses according to the International Classification of Diseases, Ninth Revision. The study was exempt from approval by the Stanford University institutional review board because all patient-level NIS data were publicly available and deidentified. We used data from the American Medical Association to assign teaching status and calculate the number of residents for each hospital (4, 17–21).

Selection of Patients

Before beginning our analysis, we selected principal diagnoses and procedures that represented a broad variety of medical and surgical diagnoses, were associated with high mortality rates, and were common in both teaching and nonteaching hospitals. Using these criteria, we selected 20 medical diagnoses and 15 surgical diagnoses. Patients with these diagnoses made up 34% of the total number of deaths in the sample and 13% of the patients (**Table 1**). We classified patients as surgical if their principal procedure code represented a major surgery and as medical if their principal diagnosis corresponded to a major internal medicine diagnosis. In addition, patients who were admitted for medical diagnoses but later had major surgeries during the same hospitalization were classified as surgical patients.

We excluded populations with low mortality rates (including all pediatric and obstetric patients) because we lacked the statistical power to detect a significant effect in these groups. We also excluded patients transferred from other hospitals or correctional facilities. We assigned teaching status to hospitals by linking American Medical Association data on residency programs to NIS data on hospitals by using American Hospital Association identification numbers. We therefore excluded patients from states that did not permit the release of hospital identifiers (roughly 35% of the NIS). To create similar pre- and postregulation populations, we limited our final sample to patients from hospitals that admitted at least 100 patients both before (January 2001–June 2003) and after (July 2003–December 2004) the regulations were implemented.

Assignment of Teaching Status and Regulation Period

Teaching status is a critical variable in our analysis. The NIS classified patients as "teaching" if the admitting hospital had any type of educational program. For example, the NIS teaching variable would have erroneously classified surgical patients at teaching hospitals as teaching, regardless of whether their primary providers were residents (under the supervision of attending physicians) or attending physicians (with little or no resident involvement). To reduce this error, we first classified all patients in each hospital as internal medicine, general surgery, urology, or orthopedics patients on the basis of their principal diagnosis. We then classified patients as teaching if the hospital had a corresponding internal medicine, general surgery, urology, or orthopedics residency program (based on American Medical Association records). In addition, in hospitals with family practice programs, we classified internal medicine patients as teaching.

For example, if a hospital had an internal medicine residency program, we analyzed all patients admitted for medical diagnoses as if they were admitted to a teaching hospital. Conversely, if the same hospital lacked a surgical residency program, we analyzed all patients admitted for surgical diagnoses as if they were admitted to a nonteaching hospital. In our model, hospitals could admit a mix of teaching and nonteaching patients depending on the hospital's mix of sponsored residency programs. However, all patients with the same diagnosis in one particular hospital would be analyzed as teaching or nonteaching unless the hospital gained (or lost) a residency program from one year to the next. With this procedure, misclassification of patients is still possible if a hospital has parallel teaching and nonteaching services. We later discuss the effect of misclassification on the interpretation of our results.

Table 1. **Included Diagnoses***

Diagnosis	Total Patients, *n*†	Total Deaths, *n*†	Mortality Rate	Sample Patients, *n*‡	Sample Deaths, *n*‡	Sample Mortality Rate
Myocardial infarction						
Anterior wall, anterolateral wall	56 359	6136	0.11	21 443	2168	0.10
Inferior wall	58 436	4225	0.07	22 211	1573	0.07
Subendocardial	215 037	12 133	0.06	86 064	4592	0.05
Congestive heart failure	699 587	28 679	0.04	247 792	10 010	0.04
Pulmonary embolism	73 992	3551	0.05	27 444	1318	0.05
Stroke	247 989	15 073	0.06	88 719	5174	0.06
Intracerebral hemorrhage	42 401	13 638	0.32	15 503	5048	0.33
Pneumonia	650 948	35 283	0.05	231 748	12 797	0.06
Hypovolemia	228 377	7792	0.03	80 488	2782	0.03
Sepsis						
Streptococcal	14 906	1841	0.12	5708	707	0.12
Staphylococcal	21 800	4452	0.20	8064	1674	0.21
Escherichia coli	32 441	2053	0.06	12 403	820	0.07
Not otherwise specified	108 554	26 404	0.24	38 594	9700	0.25
Staphylococcal pneumonia	23 707	3553	0.15	8757	1382	0.16
Chronic obstructive pulmonary disease, acute exacerbation	362 037	8155	0.02	125 626	2845	0.02
Aspiration pneumonitis	120 960	21 754	0.18	45 441	8339	0.18
Acute pancreatitis	166 516	2299	0.01	57 244	822	0.01
Gastrointestinal bleeding	115 881	9662	0.08	44 674	3745	0.08
Acute renal failure, not otherwise specified	81 535	3867	0.05	27 925	1299	0.05
Urinary tract infection	208 804	4580	0.02	72 890	1623	0.02
CABG						
1 or 2 vessels	70 120	1437	0.02	23 414	472	0.02
3 or 4 vessels	76 786	1730	0.02	24 549	502	0.02
For left internal mammary artery	26 119	444	0.02	9639	157	0.02
Abdominal aortic aneurysm	16 523	1668	0.10	5973	644	0.11
Partial small-bowel resection	33 556	2508	0.07	12 231	920	0.08
Hemicolectomy	83 990	3435	0.04	31 195	1268	0.04
Sigmoidectomy	54 635	1856	0.03	19 972	682	0.03
Cholecystectomy (open)	47 624	1064	0.02	16 136	394	0.02
Exploratory laparotomy	11 677	1884	0.16	3659	587	0.16
Nephroureterectomy	29 699	351	0.01	10 544	145	0.01
Hip fracture: open reduction, internal fixation	109 247	2530	0.02	39 469	904	0.02
Below-knee amputation	22 251	958	0.04	7656	340	0.04
Above-knee amputation	18 261	1612	0.09	6038	555	0.09
Partial hip replacement	53 434	1567	0.03	25 836	700	0.03
Aortic valve replacement	19 130	850	0.04	6896	249	0.04
All patients for selected diagnoses	4 203 319	239 024	0.06	1 511 945	86 937	0.06
All other patients in NIS	27 086 060	456 762	0.02	NA	NA	NA
All patients in NIS	31 289 379	695 786	0.02	NA	NA	NA

* CABG = coronary artery bypass grafting; NA = not available; NIS = Nationwide Inpatient Sample.
† Columns for total patients and total deaths report data from the entire NIS.
‡ Columns for sample patients and sample deaths report data from the sample of patients used in analyses.

Statistical Analysis

We used a multivariate logistic regression model to estimate the changes in mortality associated with the regulations. Changes in nonteaching hospitals after July 2003 were unrelated to the regulations and therefore reflected underlying trends. Thus, changes that occurred in nonteaching hospitals act as a control for the changes associated with regulations in teaching hospitals. To derive our estimate, we subtracted the mortality trend in the control group (patients from nonteaching hospitals) from the mortality trend in patients from teaching hospitals. This method has been previously described in the economic and medical literature as a "difference-in-differences" approach (22–24). We accounted for case-mix differences by adjusting each of our regression estimates for admitting diagnosis and all previous medical conditions included in the Charlson index except uncomplicated diabetes and peptic ulcer disease (25, 26). We included age in the regression along with indicator variables for sex, race, insurance status, year and month of admission, income quartile of the patient's ZIP code, and emergency admission.

Hospitals also have very different patient populations and staffs, which cannot be observed by using available data. We tested several models to address these differences. We tried a generalized linear mixed model, but a Hausman specification test revealed that the basic assumptions underlying this model were not met (27). We therefore used a logistic regression model that treated each hospital's unobserved effect as a distinct intercept. Estimation of several hundred variables (one for each hospital) can lead to in-

Table 2. **Baseline Characteristics of Patients**

Variable	Nonteaching Patients	Teaching Patients
Mortality rate	0.055	0.062
Length of stay, *d*	5.950	6.401
Age, *y*	70.73	69.06
Female	0.545	0.519
Primary insurer		
Medicare	0.707	0.651
Medicaid	0.069	0.083
Private insurance	0.200	0.235
Self-pay or no insurance	0.024	0.030
Metastatic disease	0.031	0.034
Congestive heart failure	0.221	0.224
Chronic renal insufficiency	0.085	0.097
Previous coronary artery disease	0.315	0.345
Previous stroke	0.045	0.043
Patients, *n*	963 916	548 029

consistent estimates, but we reduced this problem by including only hospitals with more than 200 study-eligible patients (28). In addition, we compared our results to those from a conditional logistic regression model (which does not require the estimation of multiple intercepts). We found similar odds ratios in both, but we elected against using a conditional logistic regression model because the results cannot be expressed as changes in probabilities.

Using the model estimates, we calculated the change in probability of death in patients from teaching hospitals and in patients from nonteaching hospitals. We estimated the change in mortality rate associated with the regulations by subtracting the change in probability of death for patients from nonteaching hospitals (the control group) from that in patients admitted to teaching hospitals (22, 29, 30). We also calculated the relative risk reduction by dividing the change in mortality by the pre–July 2003 death rate for teaching hospitals (22, 27).

Before starting our analysis, we chose to perform 2 primary analyses (medical and surgical patients) separately; we anticipated that the regulations would affect each group differently because of dissimilarities in the previous resident working conditions. We chose sensitivity analyses and subgroup analyses based on our primary results. We accounted for NIS sampling weights in all analyses, used 2-sided *P* values in hypothesis testing, and adjusted the variance for clustering of patients at the hospital level. We used Stata 9.2 (StataCorp, College Station, Texas) for all analyses.

Table 3. **Hospital Characteristics**

Hospital Type	Hospitals, *n*
All hospitals	550
Nonteaching	438
Internal medicine residency	106
Orthopedics residency	33
General surgery residency	52
Urology residency	28

As a sensitivity test, we used the Hochberg method to adjust for increased type I error associated with multiple comparisons within families of independent hypotheses (31). For example, in 1 family of subgroup analyses, we divided patients into 3 different age groups and analyzed the regulations separately for each group. There are 3 independent hypotheses in this family, so we used the Hochberg method to adjust the *P* values to account for this. We adjusted *P* values in similar ways for other families of hypotheses.

We tested several key assumptions underlying the regression model. First, recall that we did not directly observe whether a patient's primary provider was a resident or an attending physician. Instead, we imputed teaching status on the basis of the presence of a residency program (internal medicine, general surgery, urology, or orthopedics) at the hospital. To check the effect of this misclassification on our results, we separately analyzed patients from hospitals with fewer than 15 medical residents, between 15 and 40 medical residents, and more than 40 medical residents. Patients at hospitals with large residency programs are more likely to be cared for by residents. Thus, if misclassification is important, we should expect to find substantial differences among hospitals with residency programs of different sizes in our estimates.

Second, our approach relies on the assumption that nonteaching patients serve as an adequate control for mortality trends in the hospitalized population that are unrelated to the regulations. To test this assumption, we assumed (counterfactually) that the regulatory change took place at various times before July 2003. If our assumption is correct, we should find no association between these counterfactual regulatory changes and outcomes.

Role of the Funding Sources

The funding sources had no role in the design and conduct of the study; collection, management, analysis, and interpretation of the data; or preparation, review, or approval of the manuscript.

RESULTS

Tables 1 and **2** show the distribution of admitting diagnoses and key clinical characteristics. The relatively high mean mortality rates in both groups reflect the high risk associated with the included diagnoses. **Table 3** shows hospital characteristics, including the number of hospitals with residency programs.

Results for Medical Patients

We found that the imposition of the hours cap was associated with a 0.25% decrease in absolute risk for death ($P = 0.043$), which corresponded to a 3.75% decrease in relative risk in medical patients per hospitalization (**Table 4** and **Figure 1**, *top*).

Table 4. **Changes in Mortality in Medical Patients after Work-Hour Regulations**

Variable	Patients, *n*	Change in Mortality, %	Relative Change in Mortality, %	*P* Value
All medical patients	1 268 738	−0.25*	−3.75	0.043
Teaching hospitals				
With <15 residents	821 809	0.38*	6.11	0.51
With 15–40 residents	984 872	−0.23*	−3.63	0.28
With >40 residents	1 040 371	−0.29*	−4.10	0.08
Age				
<65 y	385 618	0.08*	2.48	0.55
65–80 y	490 030	−0.23*	−3.48	0.17
>80 y	389 585	−0.71*	−6.97	0.005
Sepsis	64 561	−2.15*	−9.32	0.022
Congestive heart failure	246 985	−0.55*	−13.17	0.010
Pneumonia	231 274	−0.68*	−10.97	0.010
Gastrointestinal bleeding	44 182	−1.42*	−15.89	0.051
Infectious disease diagnoses	377 731	−0.66*	−7.94	0.007
All neurologic diagnoses	103 952	0.01*	0.14	0.97
1998–1999 vs. 2000–2001	1 151 069	0.08	1.14	0.67
1998 vs. 1999	352 908	−0.42	−6.05	0.034
1999 vs. 2000	405 881	−0.04	−0.58	0.93
2000 vs. 2001	377 197	0.37	5.60	0.27
2001 vs. 2002	352 259	0.12	1.89	0.54

* Mortality rate for January 2001 to June 2003 minus mortality rate for July 2003 to December 2004.

The regulations correlated with decreased absolute mortality (change, −0.71%; $P = 0.005$) and relative risk (change, −6.97%) in medical teaching patients older than 80 years of age (**Figure 1**, *top*). We did not find statistically significant changes in patients 65 to 80 years of age or in patients younger than 65 years of age. We also found declines in several disparate populations, including patients with congestive heart failure, sepsis, pneumonia, and gastrointestinal bleeding. In addition, the regulations were associated with a 0.66% decline in mortality among patients admitted with infectious diseases (relative risk reduction, 7.94%; $P = 0.007$) (**Table 4** and **Figure 1**, *middle*).

Results for Surgical Patients

We did not find statistically significant changes in surgical teaching patients (change in absolute mortality, 0.13%; relative change, 3.77%; $P = 0.54$) (**Table 5** and **Figure 2**). We did not observe a statistically significant change in mortality among older surgical patients (change, 0.74%; $P = 0.16$). We found no statistically significant mortality changes when comparing surgical patients from hospitals with larger residency programs with those from hospitals without residency programs (change, 0.05%; $P = 0.66$) or with patients from hospitals with nonsurgical teaching programs (change, 0.32%; $P = 0.23$).

Sensitivity Analyses

We used the Hochberg method to adjust for multiple comparisons within independent families of results. In the first group (medical and surgical patients), we rejected the null hypothesis of no change within medical patients, but at the 10% level. Even after the Hochberg adjustment, we rejected the null hypothesis of no effect for congestive heart failure, infectious diseases, and age greater than 80 years at the 5% level. For all surgical subgroups, after the Hochberg adjustment we failed to reject the null hypothesis of no association between the regulatory change and mortality.

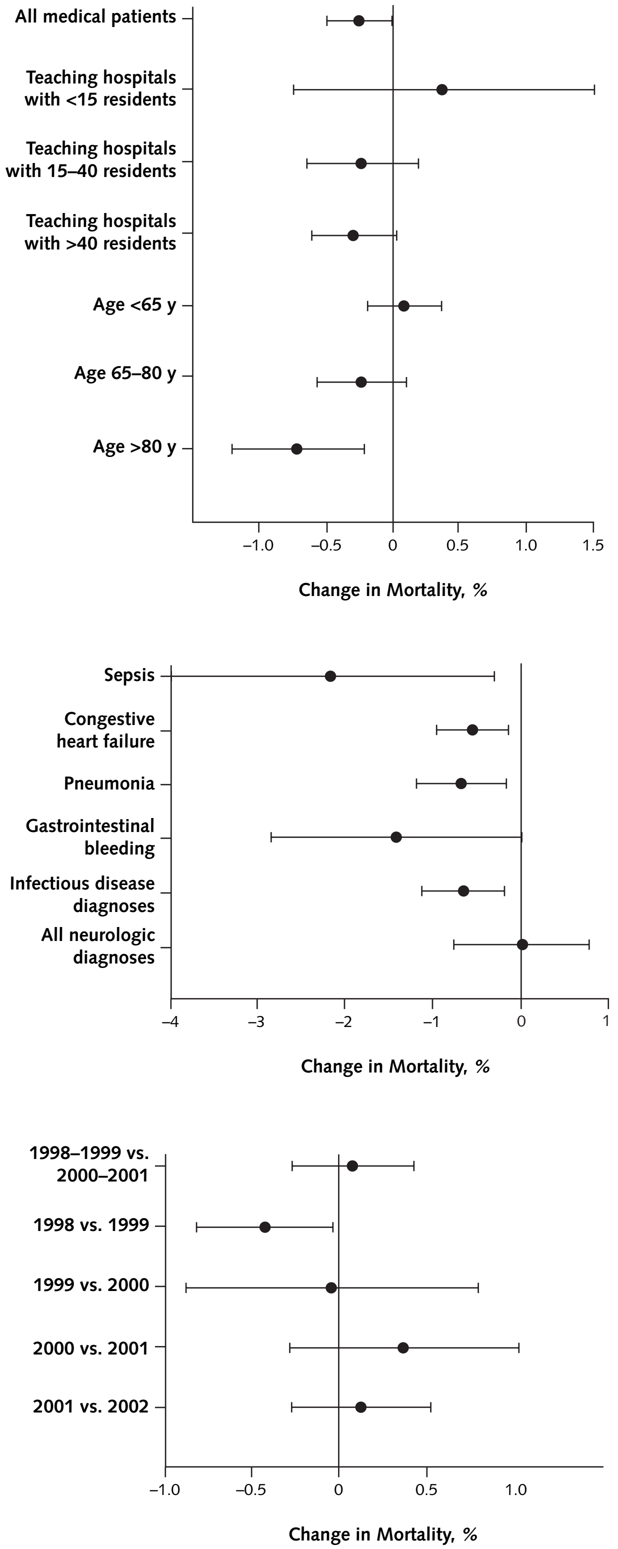
To check for the effect of misclassifying patients' teaching status, we compared mortality changes in patients from teaching hospitals of varying program sizes (<15, 15–40, and >40 medical residents) with changes among patients from nonteaching hospitals (**Table 4** and **Figure 1**, *top*). The point estimates suggested larger decreases in mortality in hospitals with more residents (and presumably fewer nonteaching patients misclassified as teaching pa-

Table 5. **Changes in Mortality in Surgical Patients after Work-Hour Regulations**

Variable	Patients, *n*	Change in Mortality, %*	Relative Change in Mortality, %	*P* Value
All surgical patients	243 207	0.13	3.77	0.54
Age >80 y	57 330	0.74	11.00	0.16
Definite surgical teaching vs. general teaching	131 477	0.32	8.88	0.23
Definite surgical teaching vs. nonteaching	180 178	0.05	1.45	0.66

*Mortality rate for January 2001 to June 2003 minus mortality rate for July 2003 to December 2004.

Figure 1. **Changes in mortality among medical patients.**



Top. Medical patients. **Middle.** Medical patient subgroups. **Bottom.** Medical patients, previous years. Error bars represent 95% CIs.

tients), although none of these differences were statistically significant (with or without the Hochberg adjustment).

For our falsification tests, we counterfactually assumed that the regulatory change took place earlier than 2003 (**Table 4** and **Figure 1**, *bottom*). There was a statistically significant decline in mortality between 1998 and 1999 (change, −0.42%; $P = 0.034$), but this decline was not significant even at the 10% level after the Hochberg adjustment. We did not observe statistically significant changes in other year-to-year comparisons or when we examined trends for the 4-year period encompassing 1998–1999 versus 2000–2001 (change, 0.08%; $P = 0.67$), with or without the Hochberg adjustment.

DISCUSSION

Despite concerns that the ACGME restrictions on work hours might have worsened outcomes, we found that the regulations correlated with a 0.25% decrease in mortality and a relative risk reduction of 3.75% for internal medicine patients at teaching hospitals. We initially hypothesized that mortality for medical and surgical patients would change after the regulations, and the data support this hypothesis for medical patients. For surgical patients, we conclude that the regulations were not associated with improved outcomes. We also examined post regulation mortality changes for various patient subgroups. After adjustment for multiple comparisons, the results were significant at the 5% level for patients admitted for congestive heart failure or infectious diseases and for patients older than 80 years of age. We regard the subgroup analyses as preliminary because these hypotheses were post hoc.

We generated these estimates with multivariate logistic regression in which we used changes in outcomes for nonteaching patients as a control for the changes induced by the regulations in patients from teaching hospitals. Various sensitivity analyses supported this approach and suggested stable relative mortality trends between patients from teaching and nonteaching hospitals in the 4 years (1998–2001) before the regulatory period.

It is striking that the regulations were associated with improved outcomes in medical patients but not in surgical patients. There are several possible explanations. First, the smaller sample size for surgical patients may have limited our power to detect statistically significant differences. Second, surgical residency programs may not have altered their working conditions substantially. Third, if the number of surgical residents remained fixed and each resident worked less, the average number of available providers would decline. Fourth, errors due to fatigue may have been counterbalanced by problems with transfers of care.

Our results differ from those of previous studies, which found no conclusive evidence that work-hour restrictions improved outcomes in multiple settings (32), including obstetric, gynecologic, and perinatal care (33) and cardiac surgery (34). Previous studies of the New York

State experience also found no effect on mortality (10) or various patient safety indicators (35). However, these studies were all conducted in smaller populations (often single academic centers), limiting both their statistical power and their relevance to national changes. In addition, whether enforcement was strict enough to have permitted statistically significant effects to be seen is unclear.

Our study has several limitations. First, we assigned teaching status on the basis of hospital characteristics because the data did not include information on individual providers. However, misclassification of nonteaching patients in teaching hospitals would blur the distinction between teaching and nonteaching patients, leading to a conservative bias toward the null hypothesis. Furthermore, we included only patients with serious and complex diagnoses in the study. In teaching hospitals, residents usually participate in the care of such patients. Hence, there were probably relatively few misclassified patients. We also observed larger improvements in mortality in hospitals with more internal medicine residents, where we would expect to find the least misclassification bias.

Second, our estimates do not represent the mortality effect of changes in exposure to resident care alone, but rather the effect of all the changes induced by work-hour regulations. For example, the regulations induced many hospitals to shift care to attending physicians (including academic hospitalists) and probably increased the number of nonteaching patients in teaching hospitals.

Third, it is always possible in an observational study that unobserved characteristics could have biased the results. However, the use of a control group (patients in nonteaching hospitals), the sensitivity analyses, the short period under consideration, and the greater improvements seen in hospitals with more internal medicine residents all argue against this bias as an explanation for our results.

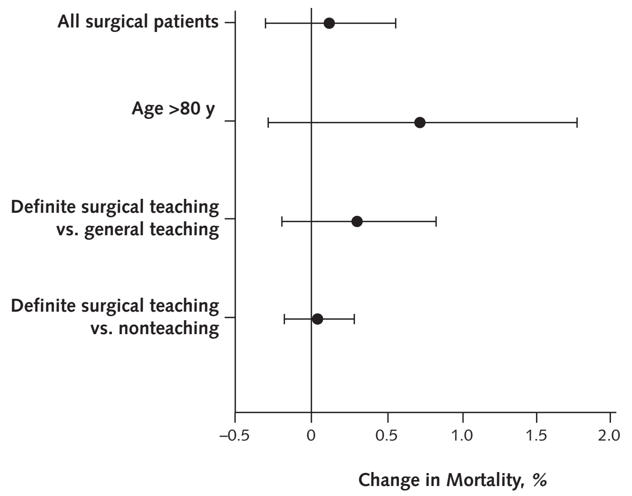
Fourth, we focused on the role of medical and surgical residents. Residents from other specialties may have played some role in the improved mortality among medical patients, but these physicians rarely have primary responsibility for medical inpatients.

Fifth, we considered only a limited set of high-risk groups, which makes it possible that mortality trends were different in other groups. We attempted to reduce this possibility by including a broad set of diagnoses, but further studies on low-risk populations (including pediatric and obstetric patients) and on larger samples of surgical patients are warranted.

Finally, data limitations permit us to say little about whether the regulations affected survival beyond discharge or how hospitals adjusted to the sudden decrease in available resident work hours.

We conclude by emphasizing findings that are worth exploring further. Our subgroup analyses suggested that the greatest associated mortality improvements were in patients older than 80 years of age and in patients admitted for infectious diseases. Future dedicated analyses of both

Figure 2. **Changes in mortality among surgical patients.**



Error bars represent 95% CIs.

results are justified. The regulations may have improved outcomes by shifting care from inexperienced residents to more experienced providers (such as academic hospitalists). If so, the regulations' long-term effect could be deleterious if physicians do not obtain sufficient experience and skills during training and make more errors after residency. Further study is needed to determine the regulations' long-term effect on both graduate medical education and patient care.

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